probably strengthen rather than obliterate the weather curve, especially when we consider the effect of increasing vegetation which would follow increased rainfall. L. E. HICKS.

Lincoln, Neb., Nov. 4, 1892.

The Moon's Atmosphere.

IN *Science* of Feb. 24, Sir Robert Ball makes application of the kinetic theory of gases to explain the absence of air from the moon. He observes that, although the mean molecular velocity of translation is less than that required by a body projected vertically from the moon to overcome the moon's attraction, "in the course of their movements, individual molecules frequently attain velocities very much in excess of the average pace," and would therefore be able to escape from the moon into space, and thus, in time, the whole atmosphere would be lost. I think a full consideration of the subject will not justify that conclusion, but that we shall be obliged to resort to some other physical laws to solve this old problem of speculation.

The kinetic theory requires all the molecules of a gas to have equal masses, equal energies, and hence equal mean velocities. This mean velocity for the hydrogen molecules at 0° C. is about 1,800 metres per second, while that of oxygen and nitrogen is about 450 metres per second, since the velocity is inversely proportional to the square root of the mass of the molecule. To overcome the moon's attraction a body must have a vertical velocity of about 2,200 metres per second. But it must be remarked that the escaping molecules, if there are such, are only those of the outer confines of the atmospheric envelope, where the mean free path of the molecules is relatively very great, as suggested with respect to the earth's atmosphere by H. Daniells ("Principles of Physics"), and the temperature of those regions is very low. If the temperature is about 68° absolute scale (— 204° C.), as assumed by some authorities, the mean molecular velocity falls to about 225 metres a second, since the velocity varies as the square root of the absolute temperature. The vertical velocity, then, or the vertical component of the velocity must be about ten times the mean velocity to balance the force of gravitation, which is not probable.

Again, if the temperature is much lower than 68° absolute, approximating the absolute zero, and the molecular velocity always obeys the law before mentioned, the velocity also would approximate zero, and of course the molecules could not escape the attraction. It appears, then, to be largely a question of the temperature of the outer limits of an atmosphere. With this in view, let us compare results on planetary bodies of different size and stage of world life. As already suggested, with respect to the earth and moon, the earth's attraction at the surface is about five times that of the moon at its surface. This, *cœteris paribus*, would require about five times greater molecular velocity of its atmosphere to escape than for that of the moon. But, if we take into account the previous history of the two bodies, it is observed that the earth was highly heated for ages after the moon had become comparatively cool, and this must have rarefied and expelled its atmosphere to great heights, and maintained a temperature in those regions which, according to the proposition under discussion, would have caused the earth to lose its atmosphere. In general, it would follow that the major planets and larger satellites would lose their atmospheres more completely while cooling than the smaller ones, unless they have correspondingly greater quantities of volatile matter in their composition than the smaller ones. And such seems to be the result. Even Jupiter, whose attraction at the surface is 2.6 times that of the earth, is believed to have an atmosphere much less extensive proportionately than the earth. Mars offers a good example of a small planet with a copious atmosphere. Its attraction is only about twice that of the moon. Why has he not lost his atmosphere? If the application of the kinetic theory alone explains the loss of the moon's atmosphere, it would require Mars to have suffered the same fate before now. Possibly we are committing the error of the Greek philosophers in treating melecules as independent masses instead of regarding them as inter-dependent centres of activity whose phenomena, as a system, constitute the qualities of matter. I do not assume to offer a solution for this complex problem, but hope rather to encourage discussion which will call out all the principles of physical science applicable to it. W. H. HOWARD.

Adrian College, Adrian, Mich., April 15.

Note on the Crystalline Lens of the Eye.

MR. MCLOUTH'S observation upon "A Peculiar Eye," as observed by him in "a domestic animal," given in *Science*, No. 531, would have been considerably enhanced in value had he recorded at the same time what that "domestic animal" was; whether it was an anserine fowl, as a duck or goose; or a gallinaceous one, as a hen, turkey, peacock, or guinea-fowl; or whether a carnivorous mammal, as a dog, or a cat; or an *Equus*, or a *Bos*, or a *Sus*, or an *Ovis*, or what not.

To the minds of some, the so-called "domestic animals" form a natural group, and even such an authority as Girard was so blind as once to propose a *special* classification for the domesticated mammals! It is not uninteresting to trace the origin of this idea, associated as it is in a way with the kindred one of man holding a place apart from the rest of organized beings.

It is only necessary to invite Mr. McLouth's attention here to the fact that the crystalline lens in the eye of man consists of *three* triangular segments, and their existence is easily demonstrated by immersion of the lens in strong alcohol, or by boiling it. The apices of these three segments are at the centre of the lens, in *front;* their bases in the circumference. Another structural feature of the lens is seen in the laminæ of which it is composed. The treatment just proposed demonstrates these also, consisting, as they do, of concentric layers, which are firm at the centre, but become softer as we approach the peripheral ones. Likewise, by thus treating the crystalline lens from the eye of a horse, we prove that it also divides into its concentric laminæ, and its *three* triangular segments. But whether this holds true in the case of all vertebrates has not, I think, been demonstrated. Very likely the crystalline lens of the "domestic animal" examined by Mr. McLouth had been submitted to a process which had a similar effect upon it as boiling or immersion in alcohol would have had, and simply exhibited its normal structure. From what I can gather from the communication of your correspondent in *Science* there was nothing abnormal about the lens of the eye he examined.

R. W. SHUFELDT.

Takoma, D.C., April 14.

The Aurora.

IN *Science* for April 7, at page 186, certain statements of mine in regard to auroral effects proceeding from the sun's eastern limb are called in question. It would have been much more satisfactory if these criticisms had given evidence of such familiarity with the subject as would be shown by the mention of even a single date on which it might be claimed that an aurora appeared in the absence of well-defined solar conditions of the character indicated. Except where specific mention is made of such individual instances, the writer proposes to refrain from discussion, which would readily become interminable as well as utterly inconclusive. Such results as those of Professor Ricco, recently announced in *Astronomy and Astro-Physics* and elsewhere, it is a pleasure to meet with and comment upon. He simply takes the case of the great magnetic storms of 1892, which were eleven in number, and studies the coincident solar conditions, especially with reference to the location of spot groups at the meridian. In seven out of the eleven instances he finds that there were such groups on the meridian, but that the magnetic effect, if it proceeded from them at all, was not felt for a varying period of from twenty-one to fifty-one hours subsequently. If, however, he had gone further and inquired what there was at the eastern limb on these dates, he would have found that there was a spot group in that location in every one of these instances without any exception whatever, and that these groups were located upon areas which were much disturbed at successive returns by rotation. Moreover, there was in these instances no appreciable retardation or variability of retardation, the magnetic storm being in progress

on the very dates when the disturbed sections were in process of being brought into view by rotation. Perhaps the most striking illustration of the whole matter in a single instance is to be found in the history of a great disturbance upon the sun in January, 1886. Upon the 12th of that month spots suddenly began to form almost precisely at the meridian and about 10° south of the sun's equator. Upon the four days following, these spots became numerous, and some of them very large, covering an enormous area, extending finally from the meridian almost half-way to the western limb. It would seem that if magnetic effects ever proceed from the sun's meridian that this, above every other, should have been a case in point. But there was scarcely any disturbance whatever and no auroras were reported from any source. On Jan. 16 and 17 the magnets were entirely free from disturbance when this great spot-group was undergoing many rapid changes and was generally in the precise location to have a terrestrial magnetic effect according to the idea which Professor Ricco attempted to work out as above described. When, however, this area was at the eastern limb, from Jan. 7 to 11, although it had not yet developed spots and was the seat of groups of brilliant faculæ only, there was an entirely different state of affairs, a great magnetic storm being in progress and auroras being reported generally from localities in high latitudes. Thus it appears that it is not faculæ in general that produce such marked effects, but faculæ in the location of areas frequented more or less persistently by spots, etc.

M. A. VEEDER.

Lyons, N.Y., April 14.

Where is the Litre?

I HAVE read Professor Mendenhall's contribution to *Science* of April 21 with surprise. I did not think it possible for so eminent a man to so entirely miss the point of any article he might condescend to read and criticise. Nor did I think it possible for so keen-witted a controversialist to so entirely forget his own argument as to admit and corroborate the very statements he set out to refute. Yet any reader of *Science* who may take the trouble to read the two articles written respectively by Professor Mendenhall and myself under the heading "Where is the Litre?" will see that both of the unlikely events in question have happened.

I invite my distinguished critic to re-peruse the paper he attacks, and to thus ascertain whether it contains any statements or contentions displaying "ignorance of the recognized principles of metrology," or whether it sets forth "certain conclusions which will generally be harmless on account of the very magnitude of their errors." If he can find any statements, contensions, or conclusions that appear to him to justify such descriptions, let him quote them in their *ipsissima verba*, and let him show in what manner they betray ignorance or error. I will then, in my turn, show the Professor to be mistaken.

This is no over-bold challenge. It is almost self-evident that Professor Mendenhall was unable to find any display of ignorance or any erroneous conclusion in my article; as, in that case, he would naturally have quoted the offending passages in justification of his severe remarks. But his only approach to quotation is worded as follows: "The sermonizing finish to the article, beginning with the sentence, 'In spite of the much lauded simplicity of metric measures,' etc., may, however, mislead a few readers whose ideas have been befogged by the perusal of the previous three pages." Such a reference is too loose, too indefinite, and too general to indicate what particular statements or conclusions are objected to; and the Professor's scornful allusion to easily-befogged readers of *Science* is, perhaps, too donnish.

And now, while leaving my critic to the digestion of my challenge, I may, without impropriety, quote some opinions that have reached me from other authorities.

1. The *Engineering News* of March 30, in an editorial reference to my paper, says: "Different enactments by legislative bodies, errors in measurement and in calculation, difference in weights between bodies weighed in air and weighed in vacuo, and difference in weights between water containing air and water freed from it have conspired to produce these variations. It is true these variations are all so small as not to affect the practical accuracy of any ordinary measurements; but for the exact work of physicists and chemists, and for some of the finer measurements of engineers, these variations are sufficient to affect the results. The moral which Mr. Emmens points is that the author of any paper or treatise claiming scientific accuracy, and dealing in quantities whose exact values may be in doubt, should preface his work with a statement of the constants adopted throughout the work. In a personal letter to us Mr. Emmens makes the further suggestion that the international congress of scientists and engineers at Chicago next summer will afford an excellent opportunity for defining anew the metric standards whose values have become most variable, thus restoring to the system the advantages of simplicity and freedom from ambiguity which it was originally intended to possess. It certainly gives good ground for criticism that in every school in the land pupils are taught that the litre is equal to the cubic decimetre, whereas, in reality, the litre is about 0.1 cubic inches larger than a cubic decimetre, the exact variation depending on what value is chosen for each."

2. Professor De Volson Wood, of the Stevens Institute, writes: "Your article in *Science*, 'Where is the Litre?' is such a model of courteous discussion that I thank you for it. The closing remarks contain sentiments I often advocate, but you have done it so much more completely and in all respects so much better than I could, that I appreciate it."

3. Mr. R. A. Hadfield, of the Hecla Steel Works, Sheffield, England, whose scientific reputation is world-wide, writes: "It appears to me you have touched the weak point of the Metric system, and it was only the other evening, at a lecture on this subject, that I was aware for the first time there was a difference between the litre and the cubic decimetre. No doubt many others are in the same way, and it would therefore be specially desirable to have some common understanding on this matter."

4. Mr. Latimer Clark, F.R.S., writes: "I will see the Board of Trade with your letters. They are as anxious as you or I can be to help in such a cause, and would do anything to promote it. The Chicago conference would afford a capital opportunity for raising the question, and I will do anything required if you will point out what you recommend. The difference between the litre and cubic decimetre is simply one of popular belief and teaching, and it arises from the French Bureau having decided to adopt the bulk of the kilogramme of water as the bulk of the litre. I may perhaps add that the Warden of the Standards here has written me that he acknowledges my dictionary as correctly setting forth the values they have adopted and are employing, and he adds that he recommends the book to all enquirers on the subject."

I refrain from adducing further evidence lest I should put Professor Mendenhall in the position of the dissentient juryman who complained that "he had never before, in the whole of his life, met with eleven such obstinate fellows."

STEPHEN H. EMMENS.

Youngwood, Pa., April, 25.

Sham Biology in America.

MR. CONWAY MACMILLAN has shown more enthusiasm than discretion in his recent article. He is writing in a good cause, namely, the elevation of botany to an equal rank with zoölogy in biological teaching in universities. Biology, however, is not the science of animals and of plants, as Mr. MacMillan maintains, it is rather the science of life; and I am not aware that biology is taught in any large institution in this country without taking advantage of the fact that certain laws and principles of life are, for purposes of practical study, far better shown in plants than in animals. Plant biology is therefore extensively taught upon the lines laid down by Huxley and Martin, and on such lines we simply select the organism which best demonstrates a certain principle. If the botanists of this country allow the zoölogists to take the lead as *biologists*, that is, in setting forth the fundamental principles of life from their observations upon animals, it will naturally follow that zoölogy will occupy the leading position in the universities. Mr. MacMillan's argument should therefore be directed to the botanists and not to the zoölogists, who are in no